We would like to thank three anonymous reviewers for their comments. The answers to the questions raised in the reviews provided below. The comments of reviewers are highlighted in *italic*.

Reviewer 1

The most critical point is separating the record into a warm a cold season part. This is conducted by implementing a threshold (average d18O value of -15.5‰ for the entire record), thereby inherently presuming a d18O-temperature relationship and the absence of a trend.

We agree that the dating section should be revised to make the dating procedure clearer as was pointed by all the three reviewers. However, we think that the proposed method of dating when the border between warm and cold seasons is the 100-years mean value is the best one for this very ice core.
Accumulation at the drilling site has been investigated sporadically (see review in Mikhalenko et al., 2015). We cannot use the meteorological observations from the nearest weather stations as these stations situated at sufficiently lower elevation and belong to two different groups as discussed in section 3.1. The ice core is the only source for the information about the seasonal cycle of this parameter.
We think that the annual cycle of the isotopic composition is influenced by local temperature while interannual variations depend on the other factors. In order to better illustrate the dating methodology we will add the ammonium concentration and dust concentration profiles to Fig. 3. Layers with the high dust concentration have been precisely dated by Kutuzov et al. (2013) for the 2012 ice core. Their results show that the separation of the core into a warm and cold season part using the average value of δ18O is appropriate for this drilling site at least for the period from 2009 till 2012 that was investigated in the paper. Also, to show correlation between temperature and isotopic composition on annual scale, we will add temperature data to the GNIP data graphs on Fig. 7.
As for the linear trend that can cause errors in the dating, we also tried separation into warm and cold seasons using linear trend of δ18O. The result is shown on Fig. below. The difference between this method of separation and ours is about 0.5 per mil which is comparable with the δ18O measurement precision and is negligible given the high accumulation rate at the drilling site.

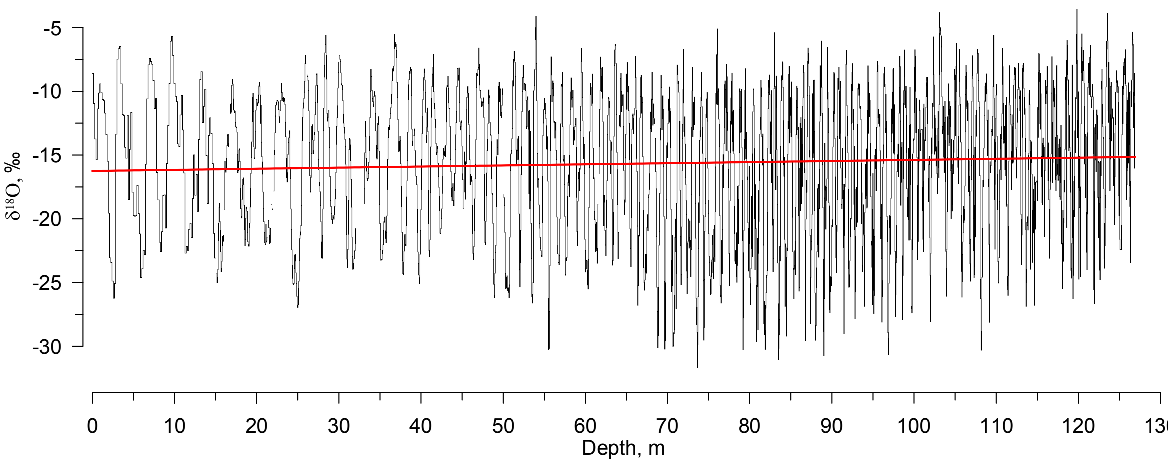

Fig. Vertical profile of δ18O with the linear trend.

So, the question is if you could investigate a longer time period (potentially showing a trend in temperature) and longer term averages to smooth the effect of year-to-year shifts in precipitation/accumulation.

Investigation of a sufficiently longer period is not feasible because of huge dating uncertainties. We will discuss them elsewhere.
Also, we tried 3-, 5-, and 7-years running means for the correlation analysis but obtained the same result. We can add these results as well.

One other point is the dating uncertainty and how you deal with that for correlation analysis with meteorological data. You specify +/- 1 year uncertainty, whereas the first publication on this ice core (Mikhalenko et al., 2015) shows a 2-years difference between annual layer counting of the stable isotope signal and the chemical stratigraphy at 106.7 m. What is correct and how to you consider this in the correlation analysis?

We agree with the comment and will correct the uncertainty as stated by Mikhalenko et al (2015). In the correlation analysis we used the dating obtained using the isotopic composition annual cycles counting. We will add this point to the text of the paper.

Obviously this is not the first publication about that ice core which is not a problem if you present other data or new analyses. Here this is not so clear and you should state it and reference it where results were already presented before. Examples are the diffusion of stable isotopes, the AWS data from the ice core site, the overlap with the shallow cores, the precision of the stable isotope analysis (0.06‰ for d18O here and 0.07‰ in Mikhalenko et al. (2015)).

The paper of Mikhalenko et al (2015) presented the ice core and the analysis done. Now we are discussing the isotopic profile of the core. Of course, some replications are inevitable in this case. We will add this point to the discussion section as well as citations of Mikhalenko et al (2015) to the data and methods section.

I wonder how the entire stable isotope record looks like. In the manuscript only the part down to 126 m out of 182 m is shown, whereas it is stated that the entire core was analysed. Why do you not focus on a longer period, for example back to 1815, since the Tambora volcanic layer gives a nice time marker, detected in most of the ice core records.

We are not going to discuss the bottom part of the core as the isotopic cycle is less prominent there and cannot be used for the dating purpose. The dating using the volcanic layers at Elbrus is complicated as Elbrus is a volcano itself. The dating of the bottom part of the core and the properties of this part like isotopic composition, chemical composition, and dust concentration will be discussed elsewhere. We focused on 100 years period because it is covered by weather observations in the region and we can obtain ice core data with annual resolution.

In the introduction you state that water stable isotopes are more sensitive to distortion because of seasonality than aerosol concentrations, which is not correct. The seasonality of aerosol-related species and the isotope signal are comparable, but the anthropogenic aerosol trend exceeds by far any temperature-driven water isotope increase during the Holocene (Wagenbach et al., 2012).

We will change this statement according to the reference provided by the reviewer

Explain why there are gaps in the data (fig. 2 and 3) and how you treated them for calculating annual averages.

The gaps came from the technical problems during the drilling operations and the analysis process. The drilling problems are thoroughly described in (Mikhalenko et al., 2015). We used the values from the duplicate core obtained in 2004 for the gap between 31.3 and 32.1 m. In case of one sample missing we considered its isotopic value to be the average between the two neighbor samples. We will add this explanation to the paper

Table 4: Include number of points n or time period for correlation analysis when they are different for the different parameters as for temperature and precipitation.

Ok, we will do this

Reviewer 2

Seasonal d18O data. The division of the data as shown in Fig 3 is largely unmotivated, except that there appears to be an annual cycle in the data. How is the distribution of seasonal accumulation? We don't know and it seems the authors have not investigated this. I suggest that a similar approach as Vinther et al. (2010) is made. I.e. investigating the proportion of the yearly accumulation to be assigned to either summer or winter depending on the coherency with meteorological observations, be it either temperature or circulation indecies. I think that before a properly motivated division of the seasons is made the effort of discussing the outcome of the analysis is not really relevant.

We will broaden the dating section as pointed before

In the introduction in general I miss a stronger representation of similar work done for Greenland although Greenland is mentioned. Many of the research questions are similar as well as the connection to atmospheric circulation patterns. See e.g. Vinther et al. 2003, Vinther et al. 2010 and Ortega et al. 2014.

We will add this to the introduction.

L57-63 here a lot of detailed processes are mention, but there are no reference to literature. Why not refer to the early isotope work by Willi Dansgaard and e.g Persson et al. 2011 on intermittency of snowfall.

We will add these references to the paper.

L169-176 I can't follow this section easily. I suppose the point you want to make is that you think diffusion has little influence on the isotope values. Did you calculate the variation of amplitude of the d18O annual cycle from top to bottom? It might "look" like there is no decease in amplitude, but what are the numbers? Another way to test if diffusion plays a role is the d-excess. Since the diffusivity of HDO and H2-18O is different there will be a phase change of d-excess with the diffusion often shifting the d-excess peak earlier in the year (depending on the annual cycle of the d-excess).

Yes, exactly, we think that the diffusion of stable isotopes does not influence the isotopic profile significantly at the part of the core that is discussed in the paper. We will add description of the calculation procedure to the section. Investigation of d-excess in this case will not add any information as the seasonal cycle of this parameter is not observed (Fig. 2).

Reviewer 3

1. If possible, it would be better to draw a dividing line in Fig.1 to separate the regions with and without a distinct seasonal variation of precipitation. This can help readers to understand some discussions in the paper.

Ok, we will add it

2. The dating is very important for the ice core study. In the section of dating, i.e. 2.1.4, authors used the mean value of the d18O of the whole dataset (-15.5 ‰ as a threshold to separate between the warm and cold seasons. This suggestion should be verified and/or confirmed by the data of d18O in precipitation at the GNIP stations around the ice core drilling site. Another way to test the effectiveness of the division of seasons in ice core is to discern if there is a consistency between the ratio of warm season accumulation rate to cold season accumulation rate (in table 3) and that of precipitation at the adjacent meteorological stations (this method was used by Wang et al (2002, Annals of Glaciology, Vol.35, 273-277) in a Himalayan ice core). Authors also mentioned that the other parameters with seasonal variational characteristics, such as dust and ammonium concentrations, were used to identify the warm/cold season in the ice core profile. It would be better to display the variations of these parameters in the Fig. 3.

The discussion of the dating methodology will be expanded. Also we will add the dust and ammonium concentration profiles to the fig. 3.

3. Authors calculated the correlation between temperature and d18O in the Lines 329-332 of the text using the 11-year running means for the different periods, and found that the correlations changes with time. If possible, authors can do this by a sliding window method used by Wang et al. (2003, Geophysical Research Letters. Vol.30, No.22, doi: 10.1029/2003GL018188) in a Tibetan ice core. Another issue is that the data series used in the paper ended in 2013, why their 11-year running means also ended in 2013 (shown in Fig. 11)?

We will reconsider these calculations according to the reviewer's suggestions.

4. The significance test in the paper should be paid much attention, especially for the datasets of 11-year and 20-year running means. The degree of freedom can be reduced sharply for the running mean datasets. For example, as for the 11-year mean data sets over the period of 1994-2013, their degree of freedom is only 2 (20/11 is about 2).

We agree with the comment and will broaden the discussion of the statistical methods used for the calculations. In the example of the reviewer the degree of freedom (N – 2n – 2, where N is number of data points and n – smoothing period) are 35 – 22 – 2 = 11 for the period from 1914 to 1928 and from 1994 to 2013, and 65 – 22 – 2 = 41. In this case the correlations discussed in the paper are still statistically significant with $p<0.05$.

5. In the paragraph, Lines 343-346, authors should present the results of the seasonal cycle of precipitation isotopic composition calculated by using the LMDZiso model, and compare that with the ice core record in one chart.

Ok, we will add it

6. When discussing the variations of δ18O in precipitation in lines 362-365, the continental effect should be considered.

Ok, we will add the discussion of the continental recycling to the section

7. In Tables 2 and 4, the period of calculation should be presented.

Ok, we will add it

8. Line 321, "in the Alps by (Bohleber et al., 2013)" should be "in the Alps by Bohleber et al. (2013)".

Ok, we will correct it

9. Line 327, "the methods described by (Bohleber et al., 2013)" should be "the methods described by Bohleber et al. (2013)".

Ok, we will correct it